mination of the nature of that phenomenon, if true, serves to confirm the statements of others who have observed similar occurrences. The confirmation, however, only extends so far, as proving that, a yellowish substance has been seen on bodies after a fall of rain. I can mention instances where sudden appearances have taken place, and would be called *yellow* rain, without the aid of ordinary rain.

The earliest case I am acquainted with occurs in Pliny,[*] who says:—

"In like manner it rained iron in Lucania, the year before that in which M. Crassus was slain by the Parthians; and together with him all the Lucani, his soldiers, of whom there were many in his army. That which came down in the rain resembled in some sort sponges; and the aruspices gave warning to take heed of wounds from above."

This records two kinds of showers, viz., iron and sponges, but they were, I believe, one shower, although I have never seen any explanation of this curious passage. An account of a few "preternatural showers," I have lately come across, seems to contain exactly the information I have been seeking to clear up this difficulty. As it is novel, and was written in apparent ignorance of Pliny's record, it has at the present time some value. It says, in attempting to explain the cause of "red snow":—

"This singular phenomenon, which has been observed in the Arctic regions, seems to be owing to the presence of oxide of iron in a state of minute division, and also of a resinous vegetable principle of an orange-red colour, belonging apparently to some lichen, of which too, perhaps, the iron may form one of the immediate principles."[†]

This explains Pliny to a certain extent; in the common ignorance of both ascribing the redness of the snow to iron rust, or oxide of iron, and the matter that remained after the dissolution appearing of a sponge-like texture, and called a sponge or sponges by Pliny, and a resinous vegetable principle of an orange-red colour by Phillips.

This I think is the most probable meaning, and is, perhaps, the earliest "yellow" shower on record. The colour, as stated by the latter, is nearly correct, as shown by the investigation of MM. De Candolle and Prevost, who discovered, microscopically, that the red snow was due to the presence of small globules of a bright red colour, which were surrounded by a gelatinous membrane, transparent and slightly yellow, and were mixed with fragments of moss and dust. "An examination of the crimson snow found by Captain Ross in the Arctic regions by M. De Candolle proved it to be identical with the Alpine red snow; the globular bodies are of a vegetable nature, and were once thought to belong to the *Uredo*, but M. Bauer disproved this, and named the plant *Protococcus nivalis*. There are cases, however, in which the presence of animalcules gives a reddish tinge to snow."[‡]

Honeydew is mentioned by Pliny (Bk. XVIII. c. 28), who states that a great many of the ancients affirmed that dew burnt up by the scorching sun is the cause of honeydew on corn.

In the *Chronicum Scotorum* is the earliest direct record of a "shower of honey" I know of.[§] It says: "A.D. 714 . . . . it rained a shower of honey upon Othan Bec. . . . ."

When it is known that any sudden appearance, giving a colour to the ground, or prominent places, or on trees, &c., is generally thought to have descended from above, this passage is quite intelligible. The "shower of honey" was nothing more than a "secretion of *aphides*," whose excrement has the privilege of emulating the sugar and honey in sweetness and purity."[‖] Some contend that it is due solely to the exudation of the saccharine juices of trees; but, feasible as this may seem, it is not sufficient to account for this phenomenon, which often extends over very large tracts of land. If the exudation is promoted by the aphides, and the dew increased by their own excrement, then this explanation is, I believe, the true one. The former view is not to be discarded without some consideration; for one observer states that, in the course of thirty years he had attended to this subject, he had never met with any honeydew which did not seem to him to be clearly referable to aphides as its origin.[¶] This view does not go counter to what I conceive to be the correct one; for exudations do take place, and the quantity of "dew" can be increased by the aphides.

There is a very curious account given in a now little known work of what was considered the real cause of "honeydew," but I will not trespass further upon the valuable space of this journal in quoting it. I give the title at foot.[*]

More can be said upon this interesting subject; and on another occasion I hope to resume the investigation, by attempting to explain the "yellow rains" of a different kind to those treated of in this letter.

JOHN JEREMIAH

43, Red Lion Street

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[*] Bk. II. c. lvi.

[†] Lectures on Natural Philosophy, 1st series, by Montagu Lyon Phillips. London, 1839, pp 47-48

[‡] Vegetable Physiology, by Dr. W. B. Carpenter, 1858, p. 580.

[§] This I take to be equivalent to "honeydew."

[‖] Kirby and Spence's "Entomology," 1867, p. 119.

[¶] Kirby and Spence, foot-note, p. 119.

[*] Rohault's "System of Nat. Phil.," by John Clarke, D.D., vol. ii, p. 217. London, 1723.

Black Rain

THE following notice of a shower of black rain, which has been sent to me by my friend Mr. G. J. A. Walker, of Norton Villa, near Worcester, though not so exact in its description as I could have wished, may call attention to the subject, and elicit a more detailed account, if in this ungenial season rain of a similar nature has fallen elsewhere. Mr. Walker's residence is about three miles south-east of Worcester, and he says, that after three or four hours of common rain on Tuesday June 6, it became suddenly dark about seven o'clock, P.M., and shortly after a rain like ink poured down for a quarter of an hour, after which light returned upon the scene. The following morning the sheep at Woodhall (an adjacent farm) appeared as if their fleeces had been dyed black; also the dog and a grey pony that Mr. Walker had out in a field close by appeared as if they had been rolling in soot or in a coal hole. The black matter brought down with the rain was of an adhesive nature, and at Littleworth, within a mile of Norton, where this rain fell into some tubs, it was observed to be as black as ink. This black rain was particularly remarked, as clear ordinary rain had been falling for some hours on the day mentioned, but had ceased an hour previously to the commencement of this black downfall. The actual rain of that evening did not extend to Worcester, but I have a note taken at my residence here at the time, that "the gloom was singular and overpowering all the evening." I regret that, going into Herefordshire the next day, I was not aware of this occurrence till some days after, and none of the black rain or the adhesive matter it brought down had been preserved for microscopical examination.

EDWIN LEES

A New View of Darwinism

I HAVE noticed that NATURE is very catholic in its sympathies, and allows all views which are not palpably absurd to be discussed in its pages, and I therefore venture to ask for some space in which to present a few of the difficulties which have been suggested by Mr. Darwin's theory of Natural Selection, and which have not, so far as I know, been as yet discussed. I have not the taste for the language nor the arguments which were used by a *Times* reviewer, and I have much too great a reverence for one of the most fearless, original, and accurate investigators of modern times, to speak of Mr. Darwin and his theory in the terms used by that very ignorant person. Approaching the subject in this spirit, and knowing how very small a section of biologists are now opposed to Mr. Darwin, I may be very rash, but hardly impertinent, in stating my difficulties.

I cannot dispute the validity and completeness of many of Mr. Darwin's proofs to account for individual cases of variation and isolated changes of form. Within the limits of these proofs it is impossible to deny his position. But when he leaves these individual and often highly artificial cases, and deduces a general law from them, it is quite competent for me to quote examples of a much wider and more general occurrence that tell the other way. In this communication I shall confine myself to Mr. Darwin's theory, and shall not trepass upon the doctrine of evolution, with which it is not to be confounded.

The theory of Natural Selection has been expressively epitomised as "the Persistence of the Stronger," "the Survival of the Stronger." Sexual selection, which Mr. Darwin adduces in his last work as the cause of many ornamental and other appendages whose use in the struggle for existence is not very obvious, is only a by-path of the main conclusion. Unless by the theory of the struggle for existence is meant the purely identical expression that those forms of life survive which are best adapted to survive, I take it that it means in five words the Persistence of the Stronger.

Among the questions which stand at the very threshold of the

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whole inquiry, and which I have overlooked in Mr. Darwin's books if it is to be found there, is a discussion of the causes which produce sterility and those which favour fertility in races. He no doubt discusses with ingenuity the problem of the sterility of mules and of crosses between different races, but I have nowhere met with the deeper and more important discussion of the general causes that induce or check the increase of races. The facts upon which I rely are very common-place, and are furnished by the smallest plot of garden or the narrowest experience in breeding domestic animals. The gardener who wants his plants to blossom and fruit takes care that they shall avoid a vigorous growth. He knows that this will inevitably make them sterile; that either his trees will only bear distorted flowers, that they will have no seed, or bear no blossoms at all. In order to induce flowers and fruit, the gardener checks the growth and vigour of the plant by pruning its roots or its branches, depriving it of food, &c., and if he have a stubborn pear or peach tree which has long refused to bear fruit, he adopts the hazardous, but often most successful, plan of ringing its bark. The large fleshy melons or oranges have few seeds in them. The shrivelled starvelings that grow on decaying branches are full of seed. And the rule is universally recognised among gardeners as applying to all kinds of cultivated plants, that to make them fruitful it is necessary to check their growth and to weaken them. The law is no less general among plants in a state of nature, where the individuals growing in rich soil, and which are well-conditioned and growing vigorously, have no flowers, while the starved and dying on the sandy sterile soil are scattering seed everywhere.

On turning to the animal kingdom, we find the law no less true. "Fat hens won't lay," is an old fragment of philosophy. The breeder of sheep and pigs and cattle knows very well that if his ewes and sows and cows are not kept lean they will not breed; and as a startling example I am told that to induce Alderney cows, which are bad breeders, to be fertile they are actually bled, and so reduced in condition. Mr. Doubleday, who wrote an admirable work in answer to Malthus, to which I am very much indebted, has adduced overwhelming evidence to show that what is commonly known to be true of plants and animals is especially true of man. He has shown how individuals are affected by generous diet and good living, and also how classes are so affected. For the first time, so far as I know, he showed why population is thin and the increase small in countries where flesh and strong food is the ordinary diet, and large and increasing rapidly where fish or vegetable or other weak food is in use; that everywhere the rich, luxurious, and well-fed classes are rather diminishing in numbers or stationary; while the poor, under-fed, and hard-worked are very fertile. The facts are excceedingly numerous in support of this view, and shall be quoted in your pages if the result is disputed. This was the cause of the decay of the luxurious power of Rome, and of the cities of Mesopotamia. These powers succumbed not to the exceptional vigour of the barbarians, but to the fact that their populations had diminished, and were rapidly being extinguished from internal causes, of which the chief was the growing sterility of their inhabitants.

The same cause operated to extinguish the Tasmanians and other savage tribes which have decayed and died out, when brought into contact with the luxuries of civilisation, notwithstanding every effort having been made to preserve them. In a few cases only have the weak tribes been supplanted by the strong, or weaker individuals by stronger; the decay has been internal, and of remoter origin. It has been luxury and not want; too much vigour and not too little, that has eviscerated and destroyed the race. If this law then be universal both in the vegetable and animal kingdoms, a law too, which does not operate on individuals and in isolated cases only, but universally, it is surely incumbent upon the supporters of the doctrine of Natural Selection, as propounded by Mr. Darwin, to meet and to explain it, for it seems to me to cut very deeply into the foundations of their system. If it be true that, far from the strong surviving the weak, the tendency among the strong, the well fed, and highly favoured, is to decay, become sterile, and die out, while the weak, the under-fed, and the sickly are increasing at a proportionate rate, and that the fight is going on everywhere among the individuals of every race, it seems to me that the theory of Natural Selection, that is, of the persistence of the stronger, is false, as a general law, and true only of very limited and exceptional cases. This paper deals with one difficulty only, others may follow if this is acceptable.

Derby House, Eccles HENRY H. HOWORTH

Ocean Currents

MR. PROCTOR concludes his letter on Ocean Currents, in NATURE for June 15, with the remark that in theories respecting oceanic circulation "the vast distance separating the Polar from the Equatorial regions must not be overlooked." Will you allow me to point out to him that in the experiment he suggests, that vast distance is entirely overlooked; that, in fact, any such experiment, with whatever difference of detail it may be performed, whether in his cylinder or in Dr. Carpenter's trough, in no way illustrates the natural condition of things, and in no way tends to answer that objection to the "temperature" theory of currents which is founded on the infinitesimal nature of the thermometric gradients. The difference of temperature between Arctic and Equatorial water is about 50° F., or 1° F. in 100 miles; or, reducing it to smaller units suitable for an experiment, is $\frac{1}{600000}$ of a degree in one foot; this, if the experimental trough is five feet long, or if the cylinder is ten feet in diameter, gives an extreme difference of $\frac{1}{120000}$ of a degree of Fahrenheit's scale. Can such a difference be represented in any experiment? I think not; but no experiment which shows a much greater relative difference can be accepted as satisfactory; for it is the infinitesimal nature of the thermometric gradient existing in the ocean that constitutes the physical objection to the temperature theory. There are other objections which I will not allude to now; but it is manifestly no answer to this one objection to show that under certain other circumstances—which bear no resemblance in degree to those of nature—hot water and cold will establish a circulation. I, for one, have, for a good many years, been perfectly well aware that they will; but I doubt if it has ever been shown that a sensible motion will result from a thermometric gradient of $\frac{1}{600000}$ of a degree in a foot.

J. K. LAUGHTON

Alpine Floras

THE fact mentioned in last week's NATURE of the absence of any Alpine flora on the Atlas Mountains, Morocco, though disappointing, is interesting. It seems to show that, during the glacial period, icebergs did not drift to the Atlas. This, however, must have been from local causes only. Mr. Wallace found a European flora on a mountain in the Eastern Archipelago—I think in Borneo—which, most probably, must have got there during the glacial period. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, June 19

A Suggestion

Is it possible that the following facts may account for the presence of *Elastrus dolosus* in the Azores? At all events, I offer them as suggestive, and for the information of Messrs. Wallace, Godman, Murray, Crotch, &c.

Lawrence Almeida, son of the first Portuguese Viceroy in India, was the first European known to visit the coast of Madagascar in the year 1506. The Portuguese circumnavigated the whole island within two years, and subsequently constantly anchored at it in their voyages to the East Indies. They also established a settlement on a steep rock on the bank of the river Franchere and near the village of Hatore, in the province of Anosi (*i.e.*, at the south-eastern extremity of the island). The valuable timber, as ebony, as well as the rich dye-woods, would be well worth taking to Europe, and thus doubtless afforded a conveyance for living larval or pupal Elaters, without any rare or improbable concurrence of events, to the Portuguese islands in the Atlantic. Many of the extremely beautiful and attractive flowering shrubs and plants would also not improbably be forwarded to Europe by the same route, in which some Elateridæ might find shelter. Is the lapse of 300 years sufficient to account for change of development? S. P. OLIVER

Southsea

HYDROUS SILICATES INJECTING THE PORES OF FOSSILS

DR. T. STERRY HUNT directed attention some time ago* to a remarkable limestone of Silurian age from Pole Hill, New Brunswick, in which I had found the

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* Proceedings of the Natural History Society of Montreal.

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